JOURNAL OF AIRCRAFT
Vol. 38, No. 3, May–June 2001

Active Flow Control—Bright Prospects and Basic Challenges

ACTIVE Flow Control (AFC) is one of the hottest research topics in aerodynamics these days. Of course, flow control is not new to aerodynamics; it is what aerodynamics has always been about—improving performance through techniques like aerodynamic shaping, wing flaps, vortex generators, etc. But these are passive techniques. The current interest centers on the term 'active,' referring to the input of small amounts of energy locally to achieve non-local changes in the flowfield with large performance gains. Basic concepts on active control date back a century or so, at least to early ideas on fluid dynamic instabilities. However, the current excitement is more about the prospects for reactive control, that is, for *adaptive* aerodynamic performance, by integrating active control concepts with state-of-the-art technologies related to sensors, micro-processors, and materials for flow actuation.

The foundations for AFC began with early studies of fluid dynamic instabilities. For example, the experimental verification of the existence of boundary layer Tollmien–Schlichting waves by Schubauer and Skramstad nearly fifty years ago used an active ribbon to initiate these slowly growing instability waves. The rediscovery of unsteady flow visualization in the 60's and 70's, followed by quantitative measurement techniques like conditional sampling, triggered a generation of thinking, experimentation, and funding for research related to coherent structures in turbulent flows. Pioneering experiments on canonical turbulent flows like jets, wakes and mixing layers continued through the 80's and early 90's showed the potential for altering mean flow properties through active forcing of intrinsic instabilities. This gave impetus for the emergence in recent years of AFC as a major research field in fluid dynamics.

In 1985, the AIAA initiated a series of conferences on Shear Flow Control, largely to provide a forum for discussion of expanding AFC research in areas like turbulent boundary layer drag reduction, boundary layer separation, aerodynamic noise, mixing processes and many other intrinsically unsteady flow processes. Since 1990, developments in adjacent fields like Micro Electromechanical Systems (MEMS) and materials like piezo-ceramics have led to major progress in sensors and actuators. This has led to the emergence of innovative actuator strategies, like synthetic jets, that have demonstrated startling control authority, at least in low speed flows. Several papers that follow this introduction describe further evolution of these strategies.

Together, the papers in this volume provide a partial snapshot of the state-of-the-art. Some address recent advances in actuators, including some potential applications of MEMS actuators, applications of synthetic jet actuators, micro blowing techniques, wall motion, and other techniques that provide oscillatory injection of momentum (or vorticity). While they focus largely on the technology itself, the papers also provide insights into current thinking by the research community on potential applications. System applications envisioned in the papers include centrifugal compressors, micro-air vehicles, rotorcraft, and high angle of attack maneuvering fighter aircraft.

From a broader perspective, the list of potential aircraft-related payoffs for AFC is long, and includes longer range, bigger payloads, greater maneuverability, stealth, lower maintenance, and improved cost effectiveness. But notice the qualitative nature of these benefits. Ironically, in contrast to the precision typical of the technical community, qualitative terms like enhance, reduce, delay, suppress, alleviate, etc., are deeply imbedded in the language used to describe the benefits of AFC. However, managers, and more to the point, *investors*, want quantitative measures to help assess the potential of AFC for real systems.

The challenge of transitioning AFC from research to application is perhaps best characterized by the term *realizability.* What can we really do with AFC, to what extent, and how much will it cost? Can we really make AFC work in the context of real systems, real manufacturing and real operations, both military and commercial? The technical community is beginning to embrace these questions, and systems engineers are taking notice of the possible advantages of AFC to address specific performance objectives. Actually, some successful near term payoffs seem quite likely, and the prospects for realizing system-level benefits from AFC appear bright. Success here will help to establish AFC as more than a patch for problems with current designs. The long-term hope is that AFC will provide a new design tool for future aeronautical systems.

Despite this very positive outlook, there is a continuing need for new ideas, and there is a tremendous opportunity for more fundamental research on one aspect of the problem that may be a forgotten frontier. The current excitement generated by advances in sensors and actuators has tended to obscure the central research issue concerning flow control architectures and algorithms. Relatively little attention has been paid to this multidisciplinary problem. The details depend on the particular application in mind, but a general observation can be made: Given the highly nonlinear, transitory, and distributed nature of many of the flowfields to which AFC might be applied, the issues of what information is needed; when and where; and how it is fused, processed, and converted into flow control commands are crucial. Realizing the ultimate potential for *adaptive* flow control may hinge on a better understanding of this generalized problem.

The 'generalized' flow control problem is one of high dimensionality and complexity. This is a classical situation where insistence on detailed precision and predictability leads to intractability. Fundamental research is needed to find alternative approaches. It is interesting to note that biological systems have found ways to solve such problems. Consider walking, for example. If, in order to walk, the human brain required the levels of precision that our engineering minds seem to demand, then walking might be an insurmountable problem. The robotics community has also wrestled with this dilemma. A case can be made, by analogy with biological systems, that fundamental research is needed to explore ways to admit more uncertainty (and flexibility) in our approaches, and to explore adaptive techniques that allow control systems to learn by experience. Surely, the algorithms and the sensors and processing architectures developed by biological systems are at least as important as their control effectors. Somehow, the flow control research community may be missing very workable approaches already discovered by nature.

It is entirely possible that adaptive flow control should be viewed as a particular example of the more general problem of intelligent systems. If so, a fresh approach to fundamental, multidisciplinary research involving elements of fluid dynamics, biology, robotics, and intelligent systems may be needed. Traditionally, the usual approach to multidisciplinary research has been to assemble teams of experts from each discipline, and then to work the interface problem. This is a lumped element approach. It is expensive and slow, paced by the problem of communicating across the interfaces, and hampered by the propensity of individuals to pursue their independent interests. But it does work, to a degree. Progress comes when individuals become conversant in the relevant disciplines (or perhaps when their students do). In other words, progress seems to depend on expanding an *individual's* knowledge into adjacent fields. Perhaps progress in *adaptive* flow control would become more affordable, and more rapid, if the boundaries between fundamental research areas were less sharply defined, and if more basic research were conducted by a flow control community more aware of recent progress and prospects in intelligent systems, especially in *soft computing* approaches inspired by biological systems.